of a force perpendicular to the line joining two elements. If in any particular case the mutual force between two elements *did* act along the line joining them, all the expressions of the form (5) would vanish; but the point to which I particularly wish to call attention to is that *the principle of angular momentum cannot be deduced from Newton's laws, but a further hypothesis is necessary.*

We have not yet considered Newton's first law, and if we discard the hypothesis that the mutual action between two elements consists of a force acting along the line joining them, the next step is to inquire whether the first law will assist us. We have already pointed out that all the molecular forces[1] may be compounded into a wrench. Now Newton's first law asserts, in effect, that when the mass is at rest or is moving uniformly in a straight line, this wrench is zero; but the first law is limited to these two cases, and asserts nothing with regard to what happens when the mass possesses *acceleration*, and it is easy to see that when the mass is in either of the states contemplated by Newton's first law, the molecular forces are not the same as when it is in other states. For example, the molecular forces exerted across any section of a pendulum rod, which is oscillating, are not the same as when the rod is held at rest in any of the positions which it assumes during its motion. Newton's first law does not therefore help us. We have, however, proved by Newton's second and third laws that the *force* constituent of the wrench due to molecular forces is zero; and if we assume that the *couple* constituent is also zero, the sum of all such terms as (5) will vanish, and (4) becomes

$$\Sigma m(x\ddot{y} - y\ddot{x}) = \Sigma(x\mathrm{Y} - y\mathrm{X}) \;.\;.\;.\;.\;.\;. \;(6)$$

which is the analytical expression for the principle of angular momentum.

The latter principle may therefore be deduced from Newton's laws with the aid of one or other of the following additional hypotheses, viz. :—

(*a*) *The molecular action between two elements consists of a force acting along the line joining them.* Or,

(*b*) *The resultant couple due to molecular action is zero, whether the mass of matter be at rest or in motion.*

We must lastly consider the principle of energy.

Assuming that the bodily forces have a potential, we easily deduce from (1) and (2)—

$$\tfrac{1}{2}\frac{d}{dt}\Sigma m(\dot{x}^2 + \dot{y}^2 + \dot{z}^2) + \frac{d\mathrm{V}}{dt} = (f_{12} + f_{13} + \ldots)\dot{x}_1$$
$$+ (g_{12} + g_{13} + \ldots)\dot{y}_1 + \ldots + (f_{21} + f_{23} + \ldots)\dot{x}_2 + \ldots . \;(7)$$

whence if T denote the kinetic energy, and F denote the right-hand side of (7) the equation becomes

$$\frac{d\mathrm{T}}{dt} + \frac{d\mathrm{V}}{dt} = \mathrm{F}. \;.\;.\;.\;.\;.\;.\;.\;.\;.\; (8)$$

The function F is an unknown quantity which it is impossible to determine without making some special hypothesis respecting the constitution of matter or the law of molecular force. We shall presently show that F is zero when the matter is rigid. In the case of a viscous liquid it can be shown by means of the general equations (which depend upon the principles of linear and angular momenta, combined with a certain hypothesis concerning molecular action) that F is the dissipation function; whilst, in the case of an elastic solid it can similarly be shown that − F represents the time variation of the potential energy due to strain. That F is zero when the matter consists of a rigid body can be proved as follows:—Let ω_1, ω_2, ω_3 be its component angular velocities about the axes; then

$$\dot{x}_2 = \dot{x}_1 - (y_2 - y_1)\omega_3 + (z_2 - z_1)\omega_2$$

and the terms in F depending on the mutual action of m_1 and m_2 become

$$(f_{12} + f_{21})\dot{x}_1 - f_{21}(y_2 - y_1)\omega_3 + f_{21}(\mathrm{Z}_2 - \mathrm{Z}_1)\omega_2$$
$$+ \text{ similar terms in } g \text{ and } h.$$

This may be written—

$$(f_{12} + f_{21})\dot{x}_1 - \omega_3\{f_{21}(y_2 - y_1) - g_{21}(x_2 - x_1\} - \ldots$$

By the third law $f_{12} + f_{21} = 0$, and also by either of the subsidiary hypotheses (*a*) or (*b*) all the expressions of the form

$$\omega_3\Sigma\{f_{21}(y_2 - y_1) - g_{21}(x_2 - x_1)\}$$

vanish; whence F = 0, and (8) becomes

$$\mathrm{T} + \mathrm{V} = \text{const.},$$

which is the analytical form of the principle of the conservation of mechanical energy.

A. B. Basset.

The Artificial Formation of the Diamond.

With your kind permission I wish to make a few observation on Prof. Joly's interesting letter in your issue of March 22. in which he concludes from a consideration of the curve representing the expansion of diamond by heat that "the diamond is a form of carbon which has been subjected to high pressure when crystallising," and in which he remarks that these theoretical considerations gave rise to experiments which he only laid aside finally upon hearing of M. Moissan's success. He also says: "M. Moissan has shown that the *added condition* of high pressure has rendered a method previously unsuccessful *now for the first time successful.*" (The italics are mine.) Prof. Joly here makes a claim for M. Moissan which I must say, in justice to that gentleman, he has never made for himself.

If Prof. Joly will kindly turn to my paper on the "Artificial Formation of the Diamond," in the *Proceedings* of the Royal Society, No. 204, 1880, he will find that pressure is *not* an "added condition," and that M. Moissan has *not* "rendered a method hitherto unsuccessful for the first time successful."

Further, he will find that his theoretical deductions were practically demonstrated by me fourteen years ago, and I then described experiments showing that carbon, set free by metals under great pressure, was denser and harder the higher the pressure, and my experiments culminated in the preparation of minute quantities of carbon identical in hardness, density, and action on polarised light with natural diamond.

M. Moissan has done no more, but he has devised a process of great ingenuity which does not entail the danger and expense of mine, and which can be repeated with much greater chance of success.

If Prof. Joly will turn to my paper, he will find that, like Moissan, I obtained my diamond in conjunction with a fused mass containing iron, and fused under extreme pressure. Again, in the *Proceedings* of the Royal Society, of June 14, 1888, he will find that the Hon. C. A. Parsons there describes experiments which produced true diamond (thoroughly corroborated since the publication of his paper), and he proved conclusively that pressure is absolutely essential; and as his method is one which lends itself to the treatment of carbon in quantity at the necessary temperature and at any pressure, it is of much greater practical importance than Moissan's method.

I have the highest admiration of Moissan's work, not only with his electric furnace, but in the several fields in which he has distinguished himself; and as he has courteously admitted that I was the first to define the conditions and actually prepare artificial diamond, I feel constrained to point out that both Parsons and I had fully enunciated the conditions which Prof. Joly attributes to Moissan.

I quite agree with Prof. Joly's concluding paragraph, and my researches led me about a year ago to the construction of an apparatus capable of producing pressures up to forty tons on the square inch, in which I have good hopes of being able to melt carbon in quantity and produce diamond by fusion instead of solution.

Carbon, at ordinary pressures, passes directly from the solid to the gaseous state, and only under enormous pressure can it be made to pass through the liquid state.

It would be premature for me to say more at present, but I hope you will kindly allow me to put these facts on record, as they seem to have been forgotten even by those who are working in the field.

J. B. Hannay.

Cove Castle, Loch Long, N.B., March 26.

I was in hopes that the limited claim for priority which I made for M. Moissan would have secured me from controversy as to the claims of previous workers. Although well acquainted with the paper to which Mr. Hannay refers me, I had never derived from it the idea that Mr. Hannay attributed his results to the crystallisation of carbon out of a metal.

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[1] It seems hardly necessary to suppose that the mutual action between two elements consists of a *couple* as well as a force; for even in the case of magnetized matter, we may regard each element as the limit of a pair of positive and negative ones which are rigidly connected together, and we may deduce the theorem that the mutual action between two magnetic elements consists of a couple by considering the forces between each pair of simple elements.

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While I regret having incorrectly inferred Mr. Hannay's claims, readers of the paper to which he refers will, I think, perceive that the error was not without some justification.

Trinity College, Dublin. J. Joly.

The New Comet.

The comet discovered here on March 26 was re-observed on the 27th, 28th, 29th, 30th, 31st, and April 1st, 2nd, and 3rd, and its places on four nights were determined as follows :—

1894	G.M.T.		R.A.			Decl.	
	h.	m.	h.	m.	s.	°	′
March 26	9	30	9	54	37	+32	13
27	10	30	9	58	32	+31	38
30	9	50	10	9	12	+30	1
31	11	15	10	12	48	+29	27

The motion is becoming slower, whence it may be inferred that the perihelion passage occurred some time ago.

The comet is small and faint, with a stellar nucleus of about 12th magnitude and a short fan-shaped tail. It was discovered with a 10 inch reflector and comet eyepiece magnifying thirty times. To my eye the comet is now decidedly more obvious than it was when first seen, but this may either be due to more favourable atmosphere, or to the fact that greater familiarity with an object is apt to render it plainer.

Bristol, April 4. W. F. Denning.

Sun-spot Phenomena and Thunderstorms.

A small portion of that most interesting field of research again dealt with in Nature (vol. xlix. p. 503), perhaps requires to be trodden with caution. Of course all *a priori* reasoning as to probabilities of a connection between solar physics and the occurrence of thunderstorms may be laid aside, and it does not seem that writers on the subject have fallen into that kind of anti-scientific error. At the same time, we readily admit that a connection between solar activity and thunderstorm phenomena exists. But having devoted a little time to the consideration of the question, I may be permitted to make a remark. In the first place, so far as our observations at present extend, it is quite impossible to find a distinct relation of time between prevalence of thunderstorms over our planet and solar periodicities. In the second place, thunderstorms have been classified, and much require further classification (to which, by the way, I am just about to contribute a few results of study). Artificial or conventional classification would by no means be an object of pursuit to Dr. Veeder. Natural classification does not seem to bring us now at all near to the connection which we might anticipate. In fact, it seems to me that in reference to the thunderstorms mentioned in Nature for March 29, 1894, natural classification leads us away from the connection.

April 2. W. Clement Ley.

A Lecture Experiment.

The following experiment, to illustrate the anomalous contraction and expansion of water due to decrease of temperature, I have found to be very striking :—

A large test tube is fitted with a cork, through which a glass tube passes just far enough to allow a rubber tube to be attached. The rubber tube should be long enough to reach to the bottom of the test tube. Close the lower end of the rubber tube, and fill it and the glass tube with mercury up to a little above the top of the cork. Fill the test tube with water, insert the rubber tube, and cork and press the latter firmly in place, taking care that no air-bubbles are imprisoned between it and the water. The pressure of the cork against the surface of the water will cause the mercury to rise in the glass tube. Place the apparatus thus prepared into a freezing mixture of ice and salt. The mercury will fall slowly in the tube until the water has attained its maximum density, remain stationary for a moment, then rise on the further cooling of the water, and at the instant of freezing will make a rapid movement upward.

Armour Institute, Chicago, U.S.A. J. C. Foye.

Centipedes and their Young.

The members of the Trinidad Field Naturalists' Club will be glad if any of your correspondents can throw additional light upon the following facts in the history of centipedes, which have recently come under their notice :—

On September 17, 1892, Mr. Charles Libert, of this town, sent to Mr. R. R. Mole a centipede (*Scolopendra prasina*) which enclosed in a circle formed of the fore part of its body a circular mass of young centipedes about the size of a half-penny, and about quarter of an inch thick. The young ones were quite white. The old centipede was very vicious. The centipede and the young ones were exhibited at a conversazione of the Victoria Institute the same evening. The old centipede did not alter her position at all, and on the 21st was packed up for transmission to the Gardens of the Zoological Society, London. Dr. Sclater wrote, on October 27, to Mr. Mole stating that the centipede was dead on arrival, and only one young one could be found in the box. Mr. Libert informs me that he has once or twice found young centipedes clinging to various parts of the body of an adult. Mr. T. D. A. Cockerell (late of Jamaica), of whom inquiries were made, said this habit was new to him.

At a meeting of the Trinidad Field Naturalists' Club on July 7, 1893, the President (Mr. Caracciolo) exhibited a sketch of a centipede carrying its young between the legs of the anterior twelve segments of its body. He stated that he received the centipede, from which the sketch was made, from Mr. Guiseppi on June 20. The creature protected her young in this manner until June 25, when she altered her position, and lay flat over them. On June 30 she left them, "but kept an eye on them." When undisturbed the young centipedes formed a heap, in which they remained for four days. They then gradually began wandering away from the heap, one by one, in search of food. There were about 140 young ones altogether.

At the meeting of the Club on February 2, 1894, Mr. Potter said he had been told by Mr. S. W. Knaggs that he had recently found a centipede coiled up spirally on itself. On attempting to uncoil it a number of pellets of small size fell from its under-surface. These bore the appearance of eggs. He subsequently found others clasped by the numerous legs against the creature's under-surface. The pellets, or eggs, were situated all along the under-surface of the body, and dropped from it on its being uncoiled.

Several text-books and works on natural history have been consulted with the view of finding out more about this interesting habit, but without success; and in most books it is stated that centipedes lay their eggs under dead leaves, or in a dark corner, and manifest no further concern about them.

F. W. Urich.

Port of Spain, Trinidad, B.W.I., March 6.

PROF. IRA REMSEN ON CHEMICAL LABORATORIES.[1]

On January 1 the Kent Chemical Laboratory was dedicated with appropriate exercises. The beautiful building was thrown open to inspection, and many passed through its rooms expressing admiration. Its plans were explained and a general account was given of the uses to which it is to be put. Honour, "as is most justly due," was paid to the generous donor, whose name from this day forth will be intimately associated with progress in chemistry in this country. The exercises of yesterday have led by an easy step to those of to-day, and a chemist is called upon to give the Convocation address. What theme more natural to him, or more appropriate, than "The Chemical Laboratory?" It is to this theme that I ask your attention. My purpose is to treat the chemical laboratory, not from the material point of view, but in its broader aspects, as far as I may find this possible. I shall attempt to answer briefly three questions, and these are :—

(1) When and how did chemical laboratories come to be established in universities?

(2) What part have chemical laboratories played in the advancement of knowledge?

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[1] Address delivered by Prof. Ira Remsen on January 2, 1894, in connection with the opening of the Kent Chemical Laboratory of the University of Chicago, U.S.A.

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